Numerical Exercises in Chemistry. By T. Hands, M.A., F.R.A.S. (London: Sampson Low and Co., 1884.)

THIS is a neat little book of easy arithmetical exercises on chemical problems which are likely to crop up in the course of laboratory work. The examples, about 650 in number, are to some extent original and partly collected from examination papers. They extend over a good deal of the physical ground more intimately connected with chemistry, and appear to be generally of a useful character. There is no attempt at theoretical instruction beyond what is absolutely necessary for setting out a question. The first four pages are given to exercises on the metric system, after which thermometers, heat, chemical equations, &c., are dealt with. The book will be very useful for students who have got a little way into the subject, but still in the position of beginners.

An Introduction to the Differential and Integral Calculus, with Examples of Application to Mechanical Problems. By W. J. Millar, C.E. (London: Blackie and Son., 1885.)

THIS is the second attempt within a very short time to give an elementary and, as far as possible, interesting exposition of the principles of the Differential and Integral Calculus. One cannot but feel sympathy with the authors of such attempts, for, sooth to say, we often find writers on the less elementary branches of mathematics anything but good teachers or editors of students' text-books.

The present little work has the peculiarity that, being written for engineering students, its illustrations are mainly such as can best be appreciated by those who have an acquaintance with applied dynamics. It is clearly written, the examples are well chosen, and it is on the whole wonderfully accurate, considering the appearance usually made by practical men when dealing with pure mathematics. On page 7 no distinction is made between the increment of x and the square of the increment of x; on page 12 there is a faulty investigation of the rule for the differentiation of a quotient; and one or two others might be specified. These do not detract much, however, from the value of the exposition as a whole, and we cordially hope that the little book may attain its object of smoothing and rendering attractive to practical engineers the rather forbidding pathway leading to the higher mathematics.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Whole Duty of a Chemist

I CANNOT complain if the address which I delivered a few weeks ago to the Institute of Chemistry, although it received an extent of encomium for which I was quite unprepared, should meet here and there with adverse, not to say unfriendly, criticism. Naturally enough, the inculcation of sturdy self reliance is displeasing to the new apostolate of sponging upon others, though it scarcely, I think, justifies a resort to the dialectic juggle of representing the defence of one side of a chemist's duty as meant to be a deliberate expression of "the whole duty of a chemist." The Chemical Society takes cognisance of chemists in one aspect of their work, the Institute of Chemistry takes cognisance of the same individuals in another aspect; and one really need not be a conjuror, though it may put some strain on the fair-mindedness of an editor, to perceive that an address intended for the one organisation would be unsuitable for the other. That distinguished man, the late Dr. James Young, F.R.S., whom you so complacently sneer at, was not a professional chemist at all, but a manufacturing chemist. He was a first-rate manufacturer, whereas Reichenbach was but a third-rate or fourth-rate investigator, if so much; and, your opinion notwithstanding, it is commonly held that the first-rateness of the one man in his own walk more than counterbalanced whatever weight attached to the higher walk of the other. I may cite for your information Sir Frederick Abel, Dr. Frankland, Prof. Dewar, and the late Dr. Stenhouse, as being eminent professional chemists. Though of high repute in forensic circles, I am not aware of their being never heard of at the learned Societies; but I am aware that, in common with other leading chemists of the country, they have had the bad taste to be contemptuous of your own contributions to chemical science. Can it really be that this circumstance has affected unconsciously the spirit of your leading article? I would suggest, moreover, for your editorial consideration, that to supplement criticism of an author's performance with flippant insinuations as to his personal conduct and career, is hardly in accordance with the best traditions of scientific journalism; while it constitutes undeniably bad art, as implying that the production criticised did not of itself afford adequate opportunity for attack, even, of course, with an editor's happy privilege of misrepresentation. As regards the reflections so unmistakably made on myself personally, I have little fear that the irreproachable tone of your remarks will serve to suggest the measure of their trustworthiness; and will only observe "happy are they that hear their detractions, and can put them to mending." As regards, however, your disparagement of the chemical profession at large, from which, I trust, it may not suffer beyond hope of recovery, I would venture to remind you that even that other profession, of which you are so magnanimous a member, has had its calumniators; and the words of a well-known satirist of the last century are considered by some to be as applicable now as ever, that "Of all the cants which are canted in this canting world, the cant of criticism is the most tormenting." Sterne was, happily, unacquainted with the cant of scientific mendicancy, or he might have added that that also was a very fine cant in its way. WILLIAM ODLING

Oxford, December 1.

[We print the above without comment except on the two following points. Dr. Odling has entirely misunderstood the allusion to the late Dr. Young: no sneer was intended, as will be obvious on a perusal of the whole paragraph. He has also taken as personal to himself remarks made on types not individuals.—ED.]

A Stray Balloon

THE *Times* published on September 29 a short extract from the *Bermuda Royal Gazette*, communicated by me, describing the appearance of a balloon, passing over Bermuda on August 27, and which I suggested might be one of those which ascended this year in England or France, and had not been subsequently heard of. This was followed by a letter from Mr. Charles Harding, F.R.Met.Soc., commenting on the "extreme improbability" of a balloon crossing the Atlantic, and even adding that "a little practical experience in ballooning suggests it to be thoroughly impossible."

As the columns of the *Times* are hardly suitable for the discussion, may I ask that you will allow me to make public through NATURE the further information I have received on the subject.

First, the *impossibility* is, I think, disposed of by the fact that one of the balloons sent up from Paris during the siege did actually travel rather more than half the distance, having descended in Iceland, where it was found long after. We know not how long either balloon was on its course, but it would be bold to assert that if one balloon can float four or five days another cannot float ten days. We know nothing of the exceptional conditions which prolonged the buoyancy: an unusually good varnish, peculiar folding of the silk in its collapse, a film

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of ice, may all be supposed capable of checking the escape of the gas. However, we have first to verify the fact. On the appearance of Mr. Harding's letter, I wrote to the Hon. W. H. Gosling, of Bermuda, a gentleman well known for his interest in matters of scientific inquiry, and begged him to investigate the story. His reply is before me, dated November 4:—

"On Saturday, October 31, I visited the spot where the balloon was alleged to have been seen. I am convinced of the fact. The place is a high hill east of the lighthouse. The two women were accidentally out in a field near where they live. Mrs. Bassett saw the object in the sky, high up, many times higher than the light. It seemed to her under the clouds. She knew nothing of balloons, and thought a whirlwind had raised some nets from the sea, as it appeared to her an object from which nets were suspended. She fancied she saw the corks of the net hanging at the bottom." (Mr. Gosling here remarks, "No doubt the basket, or the remains of it, of the former account, with chains, were the suggestion of her husband, who did not see it.") "She called her neighbour, and they both watched its course out over the sea, south, until it disappeared from view, which would not take long, as a brisk north wind was blowing. No one else seems to have seen it, nor would these, had not one of them accidentally looked up.

"I cannot hear of any balloon having been sent up in America, but on September 17, three weeks later, a balloon impaled itself on a church steeple in Chicago, U.S. The basket contained some torn clothing, and a branch of oak, as if it had come in contact with trees. The wind here on the three succeeding days was east, south-east, south. I suppose you know of the report of the missing balloon from Paris, in July, seen afterwards in the Bay of Biscay, going west."

So far Mr. Gosling, who incloses an intelligent letter from Mr. Robert T. Bassett, husband of the first witness, giving some compass bearings.

The *Monthly Weather Review* of the United States for September, which has not yet reached England, may perhaps throw light on the probability of an object seen floating in the air over Bermuda on August 27, whether arrived from Europe or not, being transported to Chicago by September 17. The coincidence is remarkable, but I know nothing of this incident beyond Mr. Gosling's mention of it. High winds with heavy rains prevailed in the South Atlantic and East Gulf States of the American Union on August 31, and the centre of a cyclone travelling in a north-easterly direction was then off the coast of South Carolina. A balloon drifting south from Bermuda on August 27 would be caught in the south-east quadrant of such a cyclone; and if it kept afloat long enough would, in a few days, be landed in a north-westerly and then in a northerly direction. The conditions of the question oblige me to assume that it is not a physical impossibility for a balloon, with very little weight attached, to drift about for weeks; but the singularity of the occurrence calls for every investigation, and should you admit this long communication, I hope that further evidence may be procurable from Chicago. J. H. LEFROY

Par Station, Cornwall, November 23

"Evolution without Natural Selection"

BELIEVING as I do that the words of a reviewer should be final, it is with no small amount of hesitation that I pen the following few remarks on the review of my little work entitled "Evolution without Natural Selection," which appeared in NATURE of November 12 (p. 26). The curious way in which my book has been misunderstood, and my consequent endeavour to put matters in a clear and impartial light, must be my apology for taking up your valuable space. In the first place, Mr. Romanes finds fault with the title of my book; but why, it is hard to conjecture. I venture to assert that nine-tenths of the matter it contains attempt to illustrate the operation of evolution without any natural selective process, as any impartial reader must admit; consequently, I absolutely deny that I only reserved a few odds and ends of small detail which I ascribed to other agencies. I might also state that I had a reason, and I think a very good one, in confining my remarks exclusively to birds. Had I elected to cover a wider area, I could have shown that these "odds and ends," as Mr. Romanes somewhat contemptuously calls them, do not by any means exclusively apply to birds, but to species in every other department of natural history. Mr. Romanes goes on to say that "It is the very essence of the Darwinian hypothesis that it only seeks to explain the apparently purposive variations, or variations of an adaptive kind; and, therefore, if any variations are taken to be non-adaptive, *ex hypothesi* they cannot have been due to natural selection." Precisely. And it was the immense amount of what I may call non-purposive variation which forms the line of demarcation between such vast numbers of species that I have attempted to explain by other agencies when natural selection utterly fails to do so. I most emphatically deny that I ever said, or even inferred, that these variations are "for the most part rare," as Mr. Romanes leads the reader of his review to suppose. All naturalists who are in the habit of working through large series of specimens are well aware of the immense number of species whose claim to rank as such is based upon their slight variation from a dominant type. It took me five years' hard work amongst tens of thousands of specimens to arrive at the conclusions expressed in my little book; and, in my opinion, no naturalist is qualified to write on these subjects without serving such an apprenticeship. That is why, as a specialist, I confined myself to birds alone for my examples. In the face of the array of important facts which I endeavoured to chronicle, it seems strange for a naturalist of such standing as Mr. Romanes to state that these facts "may be freely presented to the anti-Darwinians." Why "anti-Darwinians," Mr. Romanes? No one but an evolutionist (and most evolutionists are surely Darwinians) would attach any importance to these "trivial variations," and consequent intergradation of specific forms. Mr. Romanes is careful to point out how Darwin himself admits that if these trivial specific characters are "really of no considerable importance in the struggle for life, they could not be modified or formed through natural selection." Now probably it is no exaggeration to say that at least one-third of the known recognised species absolutely rest on these "trivial specific characters." If they have not been evolved by natural selection, I maintain that other and as equally potent agents as natural selection have been at work. The object of my little book was to try and explain them.

A word as to the *cause* of variation. No one who understands anything at all about the theory of natural selection ever supposes that it is an original cause of variation. Mr. Romanes cannot have read my essay very closely, for had he done so he would have seen that I drew the reader's attention to this fact (*conf.* p. 49). The *cause* of variation is quite another question, and one which after all did not materially concern my treatment of the subject. Nevertheless, I alluded to the use and disuse of organs as a direct cause of variation. I would also wish to point out that Mr. Romanes is entirely in error in saying that I "everywhere speak of isolation as the *cause* of minute specific characters." All I endeavoured to show was that isolation can preserve a non-beneficial variation when it has arisen, just as much and effectually as natural selection can preserve a beneficial variation.

Did space permit, I would like to say a few words on climatic variation, and the probable times at which natural selection is most active in the evolution of species; on both which subjects Mr. Romanes unconsciously misrepresents me. My reviewer has nothing whatever to say on my treatment of sexual selection; the use and disuse of organs, inter-crossing, the local distribution of specialised forms, polar centres as points of dispersal, &c.

Mr. Romanes seems to think that my little book was written in an anti-Darwinian spirit. Nothing of the sort. On my last page but one I said, "Let it be clearly understood that not one single syllable in the foregoing pages has been written antagonistic to Darwin's theory of Natural Selection. All I have done has been to attempt to explain certain phenomena which the Darwinian hypothesis can never do, and which its supporters ought never to have attempted to make it explain." If I have not made my meaning plain, and thus left myself open to misunderstanding, it will be a source of great regret. Science and simplicity should be synonymous. In the French edition, shortly to be published, which is now being translated by Dr. Varigny, of Paris, I hope to make a few corrections and additions, which I trust may possibly render me less liable to be misrepresented in future. CHARLES DIXON

London, November 21

I FREELY admit that the impression left upon my mind after reading Mr. Dixon's essay was the same as that which was first conveyed by its title—viz. that the author supposed his work to

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